Quantitative Modeling to Link Climate Change Scenarios to Regional Hydrologic Processes and Invasive Species Impacts in the Bay−Delta System

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0175: Quantitative Modeling to Link Climate Change Scenarios to Regional Hydrologic Processes and Invasive Species Impacts in the Bay−Delta System

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The grandiose objectives are to assess the implications of past and future changes in climate, land use, and invasive plant species on regional hydrology, water operations and environmental processes in the Bay−Delta system. The plan is to apply existing models (HYDRA, SWAT, CASA), but no justification is provided that these models are appropriate or are improvements over existing models. No detailed plans for validation or uncertainty analyses are offered. Especially troublesome are statements such as "HYDRA's statewide predictions of river flows into the Delta will be used as up−stream inputs to the SWAT model's physically based predictions of future sediment transport, nitrogen and phosphorus loadings, algal biomass, biochemical oxygen demand, and pesticide flows." If only doing so was so simple. Although the investigators have solid credentials, this project is seriously understaffed and very expensive for the effort reported.

Additional Comments:

Even though linking hydrological models and completing their source inventory is a feasible approach to a very complex and comprehensive problem, the task is formidable. The modeling

proposed here is empirical, dependent on extensive use of remote sensing and GIS layers to provide the regional coverage needed for a large watershed. The authors note that HYDRA and SWAT have performed well, however, achieving such precision requires numerical adjustments that rule out the use of conceptually more meaningful models that might be more capable of future projections dependent upon capturing behavior that might shift radically outside the realm of existing data. What is the accuracy of predictions regarding highly uncertain expectations associated with the volume and distribution of precipitation under global warming, the extent of snowpack reduction in the Sierra, and the projection of invasive species responding to changes in streamflow timing, temperature and flow volume. As it is to be developed, invasive species projections are based on current incidence data. Without better causal data, such projections are likely to be naive, and unable to predict future distributions. The proposed hydrologic model development is uninspiring, the use of future climate scenarios does not properly account for uncertainties and scaling issues, the hydrologic models are unlikely to have the capabilities to realistically model operations of California's water projects, and there seems to be no clear plan to model future changes in plant species. This application has a narrowly focused team and makes minimal mention of partners other than as data sources. The project is not well designed. In Task 1 the applicants describe two separate hydrologic modeling systems, and do not outline the role of each and how they will be coupled. The proposed improvements to CASA−HYDRA are in the realm of decision support modeling, for which the applicants appear to have no experience. The proposed improvements to SWAT are model calibration and additional of extra data layers (e.g., weed cover, fertilizer application), but it is unclear how this will result in significant advances in modeling capabilities for the community at large. The statistical downscaling aspect of this work (Task 2) does not address uncertainties in global climate models, and does not address problems of local scale space−time variability important for hydrologic application of downscaled output from global climate models. Improving understanding of project operations (task 3) requires expertise that the applicants do not possess. For task 4, the

applicants do not explain how they will produce projections of future land use patterns, or how uncertainties will be modeled.

The grandiose objectives are to assess the implications of past and future changes in climate, land use, and invasive plant species on regional hydrology, water operations and environmental processes in the Bay–Delta system. The plan is to apply existing models (HYDRA, SWAT, CASA), but no justification is provided that these models are appropriate or are improvements over existing models. No detailed plans for validation or uncertainty analyses are offered. Especially troublesome are statements such as "HYDRA's statewide predictions of river flows into the Delta will be used as up–stream inputs to the SWAT model's physically based predictions of future sediment transport, nitrogen and phosphorus loadings, algal biomass, biochemical oxygen demand, and pesticide flows." If only doing so was so simple. Although the investigators have solid credentials, this project is seriously understaffed and very expensive for the effort reported.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The questions asked are excellent, but the approach is somewhat outdated. The panel felt that the models being used in this proposal were far from the best models available for this project. Given that these models will then be linked and require many data sets, uncertainties will propagate across models, and they are not addressed adequately. The project requires an inter–disciplinary team, but the applicants' expertise is not nearly broad enough to convince the panel that they can execute the range of tasks identified.

Rating: Inadequate

Technical Review #1

proposal title: Quantitative Modeling to Link Climate Change Scenarios to Regional Hydrologic Processes and Invasive Species Impacts in the Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals of this proposal are eminently reasonable and well defined. This is an ambitious proposal, but if achieved represents a major contribution to hydrologic and environmental management.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This proposal uses models developed previously as major elements in a forward projection of the effects of global warming and future urban−agricultural demand on water supply and allocation in California. These models (CASA−HYDRA for water flow from land surfaces, SWAT for water allocation among channels) are to be linked to generate revised water flow estimates under global warming scenarios. These are spatially explicit models which in turn link to remote sensing and GIS. Since empirical estimation of changes over such a large scale cannot be accomplished without a model, this proposal essentially is the only way to attack the problem.**

Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Even though linking hydrological models and completing their source inventory is the only feasible approach to a very complex and comprehensive problem, the task is a formidable one.The modeling proposed here is highly empirical, dependent on extensive use of remote sensing and GIS to provide the regional coverage needed for a very large watershed. The potential for error is large; accurate data and good statistics are required to minimize estimation errors. The authors note that HYDRA and SWAT have performed well, and I am willing to concede that estimating water volume in stream channels probably can be calibrated well enough to yield satisfactory precision. However, achieving such precision requires numerical adjustments that rule out the use of conceptually more meaningful models that might be more capable of future projections dependent upon capturing behavior that might shift radically outside the realm of existing data. Insight into model behavioral is not likely in this proposal, given the highly empirical nature of the models. I am particularly concerned about the accuracy of predictions regarding hihly uncertain expectations associated with the volume and distribution of precipitation under global warming, the extent of snowpack reduction in the Sierra, and the projection of invasive species responding to changes in streamflow timing, temperature and flow volume. As it is to be developed, invasive species projections are based on current incidence data. Without better causal data, such projections are likely to be naive, and unable to predict future**

	distributions unless they are solely a function of hydrologic events that can be accurately modeled. In any case, there would be very little validation, and thus the projections would be a very insecure base for policy.
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**My comments under 'approach' suggest that the feasibility of the proposal is quite good for the expanded hydrological network, but less certain for projections that likely will require knowledge of altered system dynamics. Finishing the combined model and validating its output is a worthwhile goal for this proposal.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**This is not applicable to this proposal.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Even without validation, the new projections are likely to be interesting, comprehensive and valuable.**

Rating	**very good**

Additional Comments

Comments	**none**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The technical expertise and the track record suggest the team is quite capable.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Given the ambitious scope of work, the budget is quite reasonable.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**My major reservation is that this proposal could turn into a modeling exercise of limited importance. It is heavily based on spatial data whose validity needs to be more strongly validated. However, if done well and validated to the maximum extent possible, it should at least be a good starting point for simulation gaming, which will enable more comprehensive water allocation and use analysis than has hitherto been possible.**
Rating	

	very good

Technical Review #2

proposal title: Quantitative Modeling to Link Climate Change Scenarios to Regional Hydrologic Processes and Invasive Species Impacts in the Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals in this project are stated clearly.** **However, the ideas in this paper seem to be about ten years out of date. The proposed hydrologic model development is uninspiring, the use of future climate scenarios does not properly account for uncertainties and scaling issues, the hydrologic models are unlikely to have the capabilities to realistically model operations of California's water projects, and there seems to be no clear plan to model future changes in plant species.**
Rating	**poor**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**No, see above. The model validation results shown by the applicants could be achieved by a ten line water balance model. The applicants do not appear to have any experience in decision support modeling, which is necessary to model operations of California's water projects. The applicants do not appear to have any experience in statistical downscaling methods, e.g.,**

	how can they account for the uncertainty in climate change projections and produce downscaled climate estimates that account for the space−time variability in precipitation at local scales. As noted above, the project seems ten years out of date.
Rating	**poor**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**No, the project is not well designed.** **In Task 1 the applicants describe two separate hydrologic modeling systems, and do not outline the role of each and how they will be coupled. The proposed improvements to CASA−HYDRA are in the realm of decision support modeling, for which the applicants appear to have no experience. The proposed improvements to SWAT are model calibration and additional of extra data layers (weed cover, fertilizer application) −− I can't see how this can result in significant advances in modeling capabilities for the community at large.** **The statistical downscaling aspect of this work (Task 2) does not address uncertainties in global climate models, and does not address problems of local scale space−time variability important for hydrologic application of downscaled output from global climate models. The applicants seem to be aware of the existence of multivariate statistical methods, but have not demonstrated that they know how to use them.** **Improving understanding of project operations (task 3) requires expertise that the applicants do not possess.**

	For task 4, the appliants do not explain how they will produce projections of future land use patterns, or how uncertainties will be modeled.
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Many of the tasks described by the applicants are technically feasible (i.e., they have already been accomplished by other modeling groups). However, as noted above, the applicants do not have the expertise required to successfully complete many of the proposed work elements.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	No valuable products are likely to result from this proposa, as other modeling groups have solved all of the proposed research questions.

	Funding this proposal may help this research group get "up-to-speed" - however, this may not help the scientific community at large.
Rating	poor

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors seem to have a strong track record in research on the carbon cycle. However, this work has little relevance for the proposed research activities.
Rating	poor

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems large for tasks already completed by other modeling groups
Rating	poor

Overall

Provide a brief explanation of your summary rating.

Comments	The applicants do not appear to have the capabilities to make a substantial progress with this project.
Rating	poor

Technical Review #3

proposal title: Quantitative Modeling to Link Climate Change Scenarios to Regional Hydrologic Processes and Invasive Species Impacts in the Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The project's objectives are clearly stated and internally consistent. The concept is timely and important to comprehensive basin/watershed management.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The project is justified based on existing knowledge. The authors do a very good job of describing the conceptual models. However, the authors' justifications on page 17 are unsupported; namely, 1) the application is from two institutions (so minimally a multi−institutional initiative, and minimal durable partnerships); 2) no explanation of joint fact−finding, and 3) two co−authors with very similar experience does not lead to interdisciplinary understanding.**
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The authors do a very good job of documenting the technical/scientific approach. Yet, they fail to document personal relationships with many of the regional or local managers or stakeholders to engage in joint fact finding for site−specific data that could be vital to validate the model.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The project’s proposed approach is adequately documented and technically feasible but of limited utility without explicit stakeholder support and participation or an active mechanism to engage them in validating or using the projects model.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**This project does not have a monitoring component.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The application focused on new and original scientific outcomes (page 17) rather than measurable products. The main product mentioned was "online (Internet) decision support tools to disseminate the modeling projections to stakeholders". It is unclear how this tool will be publicized or if stakeholders will be able to use or run the model. By itself, this passive Internet based approach is a poor method to disseminate the project's results/benefits. Essentially, the authors seem to adopt a "build it and they will come and use it" approach.**
Rating	**fair**

Additional Comments

Comments	**Double check literature cited (missing cites to CAL Energy Commission; 2004 (pg. 3) and Coe et.al 2000 9 pg. 5). The authors could strengthen their application by networking more with agencies, nonprofits and other CALFED stakeholders or better document their networking. A larger, more diverse project team could greatly strengthen this application.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The two P.I.'s clearly have extensive experience with remote sensing and remote sensing based models. Dr.**

	Potter’s c.v. showed extensive funding from NASA interspersed with some competitive funding from EPA and California Air Resources Board and the U.S. Forest Service. It was unclear how much of that NASA funding was programmatic vs. competitive. Dr. Potter also has an very commendable publications record. Mr. Klooster seems qualified but his limited c.v. did not contain sufficient information to judge. The application references very strong institutional support from NASA Ames Research Center.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is really inappropriate for two senior investigators and a few masters’ level graduate students. Also, the budget seems to depend on a great deal of internal NASA funding to be successful. I would have like to have seen a letter from NASA Ames Research Center committing to that support to document leveraging funds.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**This application has a very narrowly focused team and makes minimal mention of partners other than as data sources. Extremely passive technology transfer or dissemination of project results to stakeholders and decision makers. To be competitive this effort needs have extensive interaction with other federal, state, regional and local stakeholders with management authority. I would urge the authors to work to establish these relationships or better document them**

	in a later submission if they exist.
Rating	**good**